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REMARKS

A REVIEW OF THE REPORT

ON THE

CAUSE & EXTENT OF TYPHOID FEVER

IN

MELBOURNE

BY

WILLIAM THOMSON, F.R.C.S.

"NON RES, SED DISPLICET AUTHOR"

GEORGE ROBERTSON
MELBOURNE, SYDNEY AND ADELAIDE
1879

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PREFACE.

The subjoined extract from *The Lancet*, October 26, 1878, will form an appropriate preface to the following remarks on zymotic diseases in this colony:—

"COLONIAL VITAL STATISTICS.

"These and other statistics that reach us from Australia "from time to time, show that they are collected with "great care, and, as we suppose, at some considerable "cost. But they are, nevertheless, obviously and seriously "incomplete, because no deductions are drawn from the "elaborate and industriously compiled tables before us. "Surely the figures, before being formally submitted to "the Government, could be made to pass through the hands "of a professional expert, who, possessing a thorough "knowledge of the country, as well as of his profession "(in this case medical), could present to us some useful "lessons designed to aid our knowledge of the climate, "and thus point the way to the prevention of the most "common zymotic diseases of the Colony, and so to the "average maintenance of health."

Throughout the *Report* it has been the author's aim to draw the required deductions; with what result it may, perhaps, come within the province of *The Lancet* to determine.

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CAUSE AND EXTENT
OF
TYPHOID FEVER.

In reviewing the *Report* on the cause and extent of Typhoid Fever in Melbourne, *The Argus* is so often in error, false in issue, behind the latest researches in etiology, and altogether so illogical, as to call for remark, not to defend the *Report*, but to vindicate an important subject.

The first defect in the review occurs in the words—

> "There are some, though as yet very few, who insist "that typhoid fever is as purely a contagious disease as "small-pox."

If they were named, it would at once be seen how high their opinion stands above cavil. Among them are Budd, Parkes, Watson, Paget, Ballard, Chaumont, Maclagan, Bristowe, the River Pollution Commissioners, the Medical Department of the Local Government Board, Huxley, Tyndall, Corfield, Wanklyn, Radcliffe,

Sanderson, Farr, two of the number being teachers of military and naval hygiene at Netley and Greenwich.

These are some of the few who have urged people in England to adopt a contagionist view of typhoid fever; and their action has brought the disease under better control.

A marked contrast is found here, where, with a solitary advocate of the contagion theory, the majority are either doubters halting between extreme opinions, or rigidly theoretical anti-contagionists, while typhoid fever steadily increases.

It may, perhaps, be said that sanitary works have been carried on in England, but in Victoria neglected, and hence amelioration in the one country and aggravation in the other. But it is also certain that in late years every effort to check a local outbreak of typhoid fever in England has been guided by a discovered necessity for destroying contagion. Of this the writer of the following words was aware:—

"Most medical men now-a-days recognise a specific "element in the disease which must have due consider-

"ation. They hold, in fact, that typhoid fever must "probably be always sowed in any new locality before a "crop of cases can appear."

That is to say, when typhoid fever is "caused by exhalations from cesspools or "drains, or by use of water or milk contami-"nated with sewage," the infecting sewage is itself charged with typhoid fever virus. Until so impregnated sewage cannot give birth to typhoid fever. Decomposing organic matter is never a *nidus* for typhoid fever, nor are the gases evolved carriers of the *materies morbi*, unless specific poison has been added. Ordinary sewage has found its way into drinking water for years without doing appreciable harm; but the moment typhoid excreta polluted it, typhoid fever occurred. This discovery quickened inventiveness in sanitary engineers to cut off connection between sewage and potable water, sewer air and a breathed atmosphere. Of typhoid fever causation it is the crucial test. There can be nothing of the probable about it if, according to most medical men, as well as our ablest etiologists, the fact of contagion is

as one of the settled truths of medical science.[1]

Therefore is the familiar contention that underground drainage of Melbourne would lessen typhoid fever, by merely drying the subsoil, not supported by the experience of English communities. In those towns where a diminished typhoid fever rate followed drainage, to the more speedy removal of excretal matters, taking with them in many instances the poison of the disease in question, and not to a dried subsoil, is the diminution in the death-rate from typhoid fever especially attributed.

And yet in the review it is nevertheless argued that—

"The number of persons who hold the doctrine of

---

[1] "Direct investigation is showing that epidemics of "enteric fever are not caused by sewer air, but by other "causes, generally either by infected food or drink."—MR. BALDWIN LATHAM, "*Sanitary Engineering*," 1878.

"There is no doubt that typhoid fever is propagated "mainly, if not entirely, by the poisoning of the air we "breathe and the water we drink by excretal matters "which contain the evacuations of persons suffering from "that disease."—MR. ERNEST HART, "*Manual of Public "Health*, 1874.

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"contagion in typhoid fever in some sense is, no doubt, "increasing; but the mere fact that such differences of "opinion can be held shows that the question is beset "with difficulties, and should lead to caution in forming "conclusions, and moderation in the expression of "them."

This doubting disposition differs from the decided tone of Pettenkofer, who declares the time to have come when the weaker theory must yield. On that point, all inquirers agree.

To be consistent in moderation, we here should repeal the Vaccination Law, until every scruple of "peculiar people" be overcome, and abrogate the Contagious Diseases Act, till casuists settle an ethical problem.

In a devoted defender of the rights of the minority in politics, a less liberal attitude in sanitary science hardly appears compatible. Truth about typhoid cannot be determined by a medical plebiscite.

To a remark made in the *Report* to the effect that in every inquiry into the origin of a local outbreak of typhoid fever in England, by medical officers of the Local Government Board, the special poison of

typhoid fever was invariably traced to a prior fever case, it is demurred—

> "How Mr. Thomson can commit himself to such a "statement, in the face of the proofs to the contrary "given by Murchison, is a thing not easy to understand."

Mr. Thomson's assertion rests on reports of inquiries conducted *since* the epoch of Murchison's last edition of the classic treatise on *The Continued Fevers*. One of the most notable of the series occurred, indeed, in Dr. Murchison's own household, with the remarkable incidents of which it is rather surprising to learn that any intelligent sanitarian should remain at this time of day unacquainted. Had that startling episode happened but a year earlier, it might possibly have materially modified even Dr. Murchison's views as advocated in his new volume, and less perplexed undecided etiologists. It would be taking no great freedom with the right of personal statement to add that among those who have traced a local outbreak of typhoid fever, not to any *pythogenic* source, but to directly conveyed contagion, ought fairly to rank pre-eminent Dr. M[illegible]

To his acumen the people of London are indebted for a clear exposure of the method of poisoning their children; and to him the people of London are grateful for the good work done in their behalf. They did heartily thank him for enlightening them on the cause of a domestic calamity. For an admirably conducted inquiry, Dr. Murchison received a handsome public testimonial at the hands of the veteran contagionist, Sir Thomas Watson, who warmly congratulated his personal friend, and scientific opponent, for an achievement that certainly tended to confute his own hypothesis. The recipient was gratified, and, like the "good Kent," felt amply rewarded by being merely enabled to reply—

"To be acknowledged, madam, is o'erpaid."

Similarly is rehearsed in the review, in weary iteration, the old fallacy about the immunity from risk of typhoid fever enjoyed by residents in typhoid fever hospitals, as was in the olden time of eight or nine years ago related by Dr. Russell; but never a word is [illegible] e active medical health

officer of Glasgow has since done to prove that typhoid fever is propagated in no other way than by alvine conveyed contagion. In that view he has lately been corroborated by Mr. J. Netten Radcliffe, in an address to medical students of the Royal Naval College, giving the same explanation of the fever-hospital freedom from contagion as is given in the *Report*. Mr. Radcliffe says:—

"The discrimination of typhoid from typhus "brought to light the important fact that "typhoid fever spread from the sick to the "healthy in a fashion of its own, and thus "paved the way to a conclusion which has "exercised the greatest influence over subse-"quent etiological research. . . . . The "spreading of typhus, or of small-pox, of "scarlet fever, of measles, was connected with "exposure to the emanations given off in the "breath or from the body of the sick; but the "spreading of typhoid had definite relation "with the distribution of the excremental "discharges of the sick as they were deposited "in privies, cast upon the ground, passed into "drains and sewers, gained access to wells,

"&c., and so in numerous unsuspected ways,
"a knowledge of which is as yet unexhausted,
"exercised a baneful effect upon healthy
"persons having no immediate communication
"with the sick. Hence it began presently to
"be understood that sanitary measures, as
"regards typhoid, could be alone successful
"in proportion as they stopped the various
"routes by which the excremental discharges
"of the sick could reach, in certain hurtful
"fashions, the healthy. In other words, that
"such measures could alone be successful
"when guided by the results of medical
"research in the particular disease."

Even a reference to the undoubted greater prevalence of typhoid fever in Victoria than in England is needlessly complicated by the reviewer pitting typhoid fever in Victoria against *all* continued fevers in England.

Not the author, but his reviewer, takes *typhus*, *typhoid*, and *common continued* fevers together in comparing fever ratios in the two countries. The reviewer alone jumbles them up in bewildering conglomeration. After winnowing common in the old

country, but rare in the new, to bring typhoid to the test by itself, the author observes that "typhoid fever prevails in greater relative "proportion to population in Victoria than "in England." This carefully drawn deduction is thus traversed:—

"Proof of this statement is nowhere supplied, however, "and we have taken the trouble to refer to the 39th annual "report of the English Registrar-General for the purpose "of obtaining the figures here placed alongside of those "given in Mr. Thomson's work. The rate is for every "10,000 of population, *the deaths being from all continued "fevers, including, of course, typhus, which, though not "prevalent here, causes about* 11 *per cent. of the deaths "from fever in England*[1]:—

	Victoria.	England.
1864	4·24	9·77
1865	4·72	11·09
1866	8·33	10·05
1867	7·00	7·95
1868	4·40	9·17
1869	5·16	8·46
1870	5·86	8·04
1871	3·64	6·99
1872	4·24	6·12
1873	3·61	5·85
1874	5·89	5·81
1875	5·58	5·46
1876	4·51	4·45
1877	6·26	—

[1] The italics are not in the review.

"A comparison of these figures seems to show that the "mortality in England and Victoria does not differ greatly, "though it is little creditable to us that, with a small "population such as ours, no more can be said."

The facts of the case, as compiled for the author by the Government Statist, from the Thirty-ninth Annual Report of the English Registrar-General, are as follows:—

OFFICE OF THE GOVERNMENT STATIST,

Melbourne, 19th October, 1878.

Comparative view of the numbers of Deaths from Typhoid Fever to number of persons living:—

	Per 10,000
England and Wales.	
5 Years, 1870 to 1874	3·812
Year 1875	3·740
,, 1876	3·130
Victoria.	
23½ Years, 1853 to 1876 ...	6·818
Year 1877	6·260

If the ratio for Melbourne be compared with ratios in four large towns in England, including Croydon, which is said to be amongst the worst places in England for typhoid [illegible] the same truth in another form will [illegible]
A glance at the figures on the [illegible] find this point made plain enough.

FATAL RATIOS OF TYPHOID FEVER TO PERSONS LIVING IN BRISTOL, PLYMOUTH, LONDON, CROYDON, AND MELBOURNE, FOR THE UNDERMENTIONED PERIODS, VIZ.:—

Bristol.	Plymouth.	London.	Croydon.	Melbourne.
1869-75	1869-75	1869-75	1865-75	1867-77
Per 10,000.	Per 10,000.	Per 10,000.	Per 10,000.	Per 10,000.
3·80	3·60	2·60	5·90	7·26

The details for English towns are taken from Mr. Baldwin Latham's *Sanitary Engineering*, 1878, where the figures also illustrate other points in etiology; and the Melbourne ratio is placed beside them for ready comparison.

The ratios given in Mr. Latham's work agree with the numbers stated in *The Lancet*, September 28, ult., where it is shown that in the year 1876 the typhoid fever death-rate averaged 3·11 per 10,000 persons living in England and Wales; 2·93 in the registration districts containing the twenty largest towns; and only 2·21 in the second, third, fourth, and fifth registration divisions, the population of which is mainly rural. In Victoria, outside of Melbourne and suburbs, an annual avera[illegible]

4·09 deaths in every 10,000 persons living are referred to typhoid fever, so that the excess alleged in the *Report* is no exaggeration.

To avoid impugnment of the accuracy of the figures for Victoria, the following details have been supplied by the Government Statist. By them it will be observed that in the year 1877 there were in Melbourne 249 deaths from typhoid fever. This number gives 10 deaths to every 10,000 persons living, a ratio only once exceeded in Croydon since 1869, the date when deaths from typhoid fever were first separately entered in the Register of Deaths in England. Before that time they were mixed with deaths from typhus and simple continued fevers, thereby causing intricacy and confusion in the returns, making comparisons of different communities impossible. Exact data for comparing relative typhoid fever death-rates in England and Victoria were not obtainable before publication of the *Report;* but the accuracy of the calculation then made is now amply confirmed [illegible] figures tabulated on the next [illegible] calculated ratios, show.

DEATHS FROM TYPHOID FEVER, 1867 to 1877.

Year.	Deaths from Typhoid Fever.		
	Melbourne and Suburbs.	Victoria, outside Melbourne and Suburbs.	Total of Victoria.
1867	123	332	455
1868	126	169	295
1869	116	244	360
1870	171	245	416
1871	126	143	269
1872	112	211	323
1873	115	167	282
1874	216	254	470
1875	200	255	455
1876	160	215	375
1877	249	283	532
Total in 11 years	1,714	2,518	4,232

Ratio per 10,000 of average population of the period:

Melbourne and Suburbs	7·26
Victoria, outside do.	4·09
Total of Victoria	5·13

H. H. HAYTER, G.S.

O.G.S., 16/11/78.

But objections of another kind are taken to those numbers. It is, forsooth, insinuated that diagnostic errors multiplied cases of typhoid fever faster than deadly fever poison, and that there really *could not possibly* have been so great a death-rate as is recorded of

last year, nor of the last eleven years in Victoria. The thing may seem incredible. To such a line of argument the only fitting reply is that it begged the question, by alleging inability of medical men to diagnose the disease, or hinting inaccurate reporting. Without entertaining either alternative, it must suffice to say that any discussion of the question as it stands must rest on returns sent to the Registrar-General.

From those figures it is apparent that typhoid fever in Victoria is just doubly more fatal in proportion to population than it is in England.

The aggregate mortality is less in Victoria than in England, because of a smaller variety of diseases; but the few that do occur, such as typhoid fever, are certainly more fatal in proportion to population.

None will be longer able to say, "The "whole number of cases of undoubted typhoid "fever which have occurred in this colony is "not very great."

Again, the assertion in the *Report* that "Typhoid fever is more prevalent within the

"wide boundary of Melbourne and suburbs than "in the rest of Victoria," elicits no comment; nor does the remark, "but there are no means "of ascertaining whether it is, or is not more "morbific." The proposal to register contagious fevers, as well as deaths therefrom, is not entertained. The novel plan is yet only advocated by a few, but the stage of minority argues no demerit in a scheme that, if adopted, would indicate the extent of an epidemic better than the present crude way of calculating from mere death-rate.

Though vaguely believing in an undue fever death-toll, so "little creditable," the reviewer yet

> "Cannot acquit our author of exaggeration, however, "when he says on his first page that there have been "over 2400 deaths in the four years since he began his "inquiry, a table on the following page showing only "1832 deaths in the years 1874-77."

But, what of deaths from typhoid fever during 1878? Why are they omitted? They were included in the challenged part of the *Report;* and they ought equally to have been included in the review.

When asking the Government Statist the number of deaths from fever in England, the querist could also have learned the number of deaths from typhoid fever in Victoria reported up to the middle of 1878; and before venturing to criticise his duty was to ascertain. When the returns of that fatal period come to be finally examined they will show there has been no exaggeration.

If within the same period of four and a half years there had been even only 2350 deaths from small-pox, or yellow fever, an alarmed people would have had little ear and less patience for dilettante comparisons in the fatality of different fevers. Familiarity with an everyday evil jogs along complacently; while rarer ills would give a new sensation.

Alluding to the alleged analogy from splenic fever in cattle, it is observed that—

"If, then, it has been proved, in the case of anthrax, "that the specific contagium of a zymotic disease can "multiply both within and outside of the animal body, "analogy allows us to suppose that the same may happen "in the case of another zymotic disease, typhoid fever."

Of this assumed analogy it will be enough

to say that anthrax and typhoid are not analogues. The former is not an acute specific fever, as understood by a pathologist. Better to adhere to the analogy between typhoid and vaccinia, in which multiplication of contagium outside the body is physically impossible. In one leading particular, even that analogy is incomplete, for vaccinia is common to man and other animals, but not so typhoid fever.

The caution given in the *Report* that "the "analogy of other fevers, where infective "organisms have been found, only relatively "helps investigation; but yet may hinder by "suggesting analogy untrue," is overlooked by the reviewer in forming his forced analogy between anthrax and typhoid fever, for which there is no basis of scientific fact.

Of the most vital deduction in the book, it is said that—

"Mr. Thomson energetically denies the charge of being "an advocate of the total harmlessness of dirt, and we do "not charge him with anything so absurd; but so long as "no more can be said against it than he urges—viz., "that it breeds organisms which consume the oxygen of "the atmosphere, it will not be easy to rouse the general "body of the people and local boards to a sense of their "duty."

What matters it to science whether it can or cannot "rouse the general body of the people "and local boards to a sense of their duty"? Scientific truth is not affected by moral apathy. Nor is it the place of science to frame pious frauds to frighten bestial humanity. Its only function is to expound truth, explain fact, and guide coercing legislators. The cause of typhoid fever is a question of abstract truth. Under its consideration human lives and deaths are, "by abstraction reduced to mere units "undergoing changes as purely physical as "the setting stars of astronomy or the decom- "posing atoms of chemistry; and as in those "sciences so in this, the analysis of the "elementary facts observed in their various "relations to time and place, will shed light "on the more complicated phenomena of "national life." [1]

The etiology of fever was not invented to terrify the filthy. Science subserves no end; is ever inflexible, and never used as a secondary means of gaining an ulterior purpose. It

[1] Dr. William Farr.

would not even drain Melbourne on a false pretext, or at the bidding of an erroneous pathology.

No journal would argue otherwise about political economy or sociology. In those kindred sciences all hold that truth is truth ever; and no more may foreign motive divert the science of fever etiology.

Such adroit policy one could comprehend monks resorting to when monks were the only professors of physic; but hardly ever would one dream of hearing it announced by a modern journal not hankering after a return to the dark ages. The rugged style of a Browne or a Carlyle is too pointed for such sophistry. Both writers are notable in their exposure of "*Vulgar Errors*" for heartily eschewing sham. They have in them nothing of Machiavel, and detest the artifice of *The Prince*, as well as its insidious diction.

In quaintly written seventeenth-century books one learns how England was in the throes of a plague, the cause of which exercised physicians much as the cause of fever bothers them now, making of sanitary history an idle repetition.

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Ramazzini tells how pious fraud brought the public bath in Rome into disuse. By a similar device publicists would revive a tase for civic cleanliness in Victoria. Politic pathology is more intent on working on the mind than improving the body, hence

"Nostrorum temporum destituta est Ars Medica."

When William Harvey invented a rigid theory of the circulation of the blood, he feared to publish, because it would frighten flexible theorists of the Galenic type, and "make all mankind my enemies." To pacify men affrighted at truth, Riolan contrived an eclectic theory, compounded partly of old and partly of new, and for a while made every one friendly. In a similar spirit of compromise, some hold a hybrid theory of typhoid fever. Like every other great medical truth, that of the contagious etiology of typhoid was at first confined to the studious few; then it gradually approached the class-rooms of the schools; it is now rapidly growing the familiar doctrine of popular lecturers on public health; and when, by the normal law of progressive

development, the philosophical idea reaches metropolitan journals, the triumph of theoretical and practical sanitation will be complete.

Another obvious misrepresentation runs—

"He objects to the building of a contagious disease "hospital, quite unnecessarily, as it seems to us, even on "his own theory. His reason is that it will be 'a per- "'manent nucleus of fever germs,' a practical admission "that fevers originate spontaneously, and then spread by "contagion."

The "practical admission" is not the author's, as the reviewer's words would fain imply. Alluding to such an infirmary, the text says, "*As part of the pythogenic theory*, it will "be a practical admission that fevers originate "spontaneously, and then spread by contagion." The words "*As part of the pythogenic theory*," are omitted by the reviewer to coin a fallacy. In the plainly written, easily read passage, as it stands, there is no paradox. No proposition could be plainer than that if a house be built, and fever patients taken to it, the disease did not there begin. To perceive it needed no panoptic vision. It is altogether too plain to admit of being garbled.

The eminently practical aim of teaching

every household to destroy its own contagion, finds no favour. It

"Simply means that we should trust to the average "member of the community, ignorant or careless, to adopt "measures for isolation and disinfectation, which can only "be carried out with any hope of success by properly "trained persons with the right means at their disposal."

If the law meant every typhoid fever patient to go to a contagious fever hospital, the reviewer's pronounced contagionist plan would answer well. But, as sanitary legislators, even of extreme contagionist views, contemplate no such harsh domiciliary law, nor think to so transfer any, except ordinary hospital populations, people who have to attend to home-treated patients ought to be taught how to destroy contagion. That a single isolated case of typhoid fever neglected will be enough to poison the whole community has been freely admitted; and that the opinion is coincided in, in the review, must be inferred from its argument about a contagious disease hospital. How much more dangerous will the entire number of home-treated cases be if without domestic care, seeing that, of all typhoid

fever patients, the vast majority will remain at home. As typhoid fever is the contagious fever of which the people of Victoria are likely to have most continuous experience, it becomes especially important to explain to everybody the real nature of the fever, and make them expert in destroying its contagion. It is a very easily managed process, even to an untrained nurse who is acting under the advice of a doctor who himself recognises its necessity.

Hence the significance of the remarks on the efficiency of a simple chemical, harmless to the sick, and fatal only to the living poison, that

> "Alum has no claims to equal rank with many other "articles. It may harden animal matter, but it has never "been shown to have the germicide power possessed by "chlorine, carbolic acid, &c., and therefore its tanning "action on 'animal' bodies need not enable it to kill "'vegetable' organisms as bacteria are."

Bacteria are not known to be the cause of typhoid fever. Seldom is that fever now even assumed to have a bacterial or mycetic origin. It was a few years ago so held to arise, owing to a microscopist's mistake; and medical text-books, even of recent date, have not yet had

time to exclude the error. But a review ought not to retain what every writer now discards.

Later histological investigations tend to the deduction that typhoid fever is propagated by living poison accompanying white blood globules, escaping from congested capillary blood vessels at specially inflamed points, or ulcers, into the alimentary canal. In protoplasts amœboid life precedes structure, or may exist without ever reaching a definite morphology. By amœboid movement, the poisoned protoplast can, in the act of inoculating, enter the body through a membrane. Such living motes form pabulum for formed bacteria, but never attain the higher development. Their *metabolism* is not vegetive. Whenever bacteria appear, the individuality and virulent power of still lower protistic life must cease. Bacteria devour both force and substance in converting protoplasm into their own bodies. In short, the virus of typhoid fever, while still a living contagion, is lower in the scale of being than even a globular bacterium.

Therefore, to chemically alter inchoate life-stuff is to check morbid action, by stopping a

self-multiplying process. That effect alum can readily produce by a "*tawing*," not "*tanning*," action while animal matter is moist, and chemically unstable, before being dessicated in hot dry air, on parched ground, under burning summer or autumnal sunshine that would be fatal to bacterial life. Sunlight kills bacteria.[1] They cannot endure it. They thrive in the dark. But the sun fosters typhoid fever poison. We know positively that the more sunshine the more typhoid fever. That would not be so if typhoid fever depended on the presence of bacteridia.

The microphytes multiply in dirty water kept dark. To them sunlight and purity are fatal. But purest of pure water can best conserve typhoidine. To say so, may, by some, be considered rank heresy. It shakes the venerable text-book, wedded to an opposite belief, and unable, as Dr. Stokes said, "to unlearn early teaching," when "much "that was found in medical literature was "not written in the book of nature."

[1] Dr. Arthur Downes and Mr. T. P. Blunt, in "*Proceedings of the Royal Society*," December 6, 1877.

The tawing action of alum on a film of the outer part of a protoplast would kill its contained living matter as effectually as varnishing an eggshell would smother the internal yolk. If the virulent bioplasm be let dry in the sun, no chemical short of caustic action could destroy its vitality. Hence the importance of attacking it, while fluid, the moment it leaves the sick body; hence, also, facility and utility in convenient alum. A scientific prevention of typhoid fever must proceed, *obsta principiis*, to kill virulent life-stuff, and not wait to poison bacteria.

Alum is immediately decomposed by an alkali. The dejecta of typhoid fever are highly alkaline. It is their most remarkable characteristic. "In place of being acid, as "healthy fæces always are, they are alkaline; "they also contain abundance of the ordinary "products of the decomposition of animal "matter, in the form of ammonia."[1] They

[1] Dr. Murchison adds—"Such a condition must be fatal to the existence of an animal poison, such as t of small-pox;" but that objection is shown in *Report* to be untenable.

therefore eminently supply chemical requisites for double decomposition. The alkalinity of typhoid evacuations coincides with the presence in them of animal matter exuded from specially inflamed glandular structures forming the morbid anatomical state on which rests one modern title of the disease to be called "enteric fever," a new synonym preferred by many to the old and now familiar name of typhoid fever. When alkaline animal matter, containing the toxic agent of unknown nature, decomposes alum and is itself decomposed, the poison must be destroyed. Thus is afforded a safe, simple, scientific, and efficient method of preventing typhoid fever. Being perfect, we need no other. On the chemistry of the point, or *modus operandi* of alum as a thorough prophylactic agent in typhoid fever, Mr. Johnson writes :—

"*October*, 21, 1878.

"DR. THOMSON,

"Dear Sir,—In reply to your inquiry, there is no doubt "that common alum would neutralize and destroy for "mischief any alkaline properties possessed by the alkaline "dejecta during typhoid fever or analogous complaints. "Directly alum becomes confronted with ammonia, "generally present in infectious complaints, it is decom-

"posed. Alumina is precipitated, and in virtue of its "mordant properties, instantly seizes upon nitrogenous "organic matters it may meet with, and renders them "incapable of movement or alteration; hence our dyers "take advantage of this property to fix fleeting colours "that would otherwise perish or be easily removed from "textures when washed.

"No doubt as a disinfectant alum acts as a mordant, "and not, as in the case of chlorine and some other "bodies, as a destroyer of the poison. Both, no doubt, "are equally efficient for purpose required, and the "question then resolves itself into one of convenience "between two substances—one easily and innocuously "applied; the other generated with more difficulty and "greater annoyance to surrounding circumstances, is "also, in consequence of volatility, less enduring and "permanent than the simpler means.

"Truly yours,

"Wm. Johnson,

"Government Analytical Chemist."

That typhoid fever is not bacterial in human beings is almost demonstrable, and is negatived by discovery of blood *bacilli* in the so-named typhoid fever of the pig. That disease is not a true acute specific fever, but only an anthracoid fever. Blood *spirilli* have been found abundantly during accessions in famine fever, also anthracoid. But bacteria have not been discovered in the blood in cattle lung-fever, nor in cattle plague, acute specific fevers, of

No bacteria were discoverable in sepsin prepared for Sanderson's late experiments, though they were assumed to set up the putrefactive process that produced sepsin.[1] The virus was in particles, as all morbid poisons are proved to be, because they can be filtered or diffused out of their fluid media; but the particles are also found not bacteria. Their precise nature and form are still in doubt. Beyond the visible limit, their potential is alone determined by their pathogenic power. They must be more minute than the smallest bacteria, the micrococci, which Ewart has lately shown to grow into no higher form, as is already stated in the *Report*.[2]

If this newer view of infective power operating through poisoned contents of white globules, non-bacterial, thrown off from specially-inflamed points, seem speculative, it has at least a truer basis in morbid anatomy and pathological chemistry than the theory, or rather hypothesis, resting on the forced analogy of an imaginary vegetable cell.

[1] See "*British Medical Journal*, Jan. 5, 1878.

[2] "*Proceedings of the Royal Society*," June 20, 1878.

Animal matter, a product of disease, abounds in the liquid stools of typhoid fever; but no blood bacteria of any peculiar or specific form have yet been found by the best observers, with highest microscopic powers, in that disease.

Moreover, the peculiar proximate principle *excretin*, found, by Marcet, in the fæces of man, but not in those of other animals, and under all kinds of diet, indicates some peculiar function in the glandular tissues of the human alimentary canal, the special seat of morbid action in typhoid fever. And, as every physiology has its co-ordinate pathology, so will there be in those glands peculiar morbid states. Hence the reason why cholera and typhoid fever cannot be communicated by inoculation to the lower animals, and why comparative etiology is not available in researches into the nature or recondite causes of these diseases.

It is commonly contended that typhoid fever cannot be contagious from man to man, because it is non-communicable from him to any other animal. But it does not follow that a fever that cannot be transferred from man to other ani

is not transmissible between man and man. Nor is typhoid fever poison the only substance singular in that respect. The famous sausage-poison, so deadly to man, can be taken by many animals, such as dogs, cats, pigs, with perfect impunity. Here, then, is an animal poison purely chemical in form, in which no bacteria can be found, though some have an idea that bacteria may set going the putrefactive process forming the poison.[1] However, so little is known of its nature that it is called a product of a "modified or peculiar putridity." The nature of its action is equally obscure. Some unknown change goes on in the blood, not like that under any other form of poisoning; and, moreover, that action is in man unique. There must, therefore, exist in human blood

[1] In the production of *sepsin*, and of sausage-poison, as in the production of *indol* in artificial pancreatic digestion, the presence of bacteria or similar organisms is supposed to be essential as initiators of putrefactive or fermentive changes ending in the formation of a secondary principle; and it may eventually be found that the presence of bacteria or similar organisms is likewise essential for the production of typhoid fever virus; but as yet there are no data whatever to warrant even a remotely constructive parallel, far less a didactic assertion.

some peculiar element not present in the blood of any other animal for this poison to operate on; and if it be so in one instance, there is no reason why it may not occur regarding some other form of animal poison. If it can be predicated of glandular tissues in the human body, as deducible from trial of a purely physiological nature, that their morbid secretions are essentially peculiar and distinct, then it would follow that there must be in other animals an entire absence of the anatomical apparatus for elaborating, as well as of the normal chemical element susceptible of the toxic action of the non-bacterial typhoid poison.

But many other important etiological deductions arise out of these very relevant, though seldom thought of, considerations, in every way worthy of attention.

Thus, every substance, mineral, vegetable, or animal, that operates as a poison on any animal, also acts as a poison on man. But many substances that are poisonous to man are to some other animals quite innocuous; while various substances are poisonous to man alone. Among the latter class of bodies appear to be

sausage-poison, and the poisons of cholera and of typhoid fever.

A very capital illustration of the singular susceptibility of the human system to some organic poisons is met with in the ready liability of man to fever from malaria of the Campagna around Rome, while cattle grazing there enjoy perfect immunity. This peculiarity is well expressed in a letter from a correspondent of *The Argus*, 13th November, 1878, who writes:—

"Vast herds of bullocks and horses are bred on these "lands, and tended and managed by mounted men— "*butteri*—who ride home every night to dwellings on the "surrounding hills, out of the deadly influence of the "malaria. These men are the aristocracy of the population "of the Campagna. The wretched shepherds whose duty "it is to watch and tend the flocks of sheep are recognised "as a class much below the *buttero* in the social scale. "They are not mounted, and cannot ride home to places out "of the malaria which broods over the plain. They sleep "in little huts among their flocks, with their heads in the "smoke of a wood fire. This is a great protection from the "prowling malaria fiend, but not a sufficient one, for the "miserable shepherds of the Campagna are incessantly "victims to him. They either die or drag on miserable "and listless lives, always more or less under the influence "of fever. These men have a very much worse moral "reputation than the more fortunately circumstanced "*butteri*."

in the human body, but whose homologues maintain functional activity with structural integrity in the full-grown bodies of other animals. The former view is opposed to the evolution hypothesis, the latter harmonises with it. There certainly are ingredients in human blood, the precise nature or uses of which are yet unknown, or even unconjectured by physiologists; and their relation to the different zymotic processes is yet not advanced beyond the stage of interrogative speculation. Their clinical phenomena are unexplained.

Vague as these considerations are, they yet have a philosophic interest, as well as a direct bearing on etiology, and as such they are reverently approached in the *Report*.

But another important practical inference is directly deducible from the facts narrated in the Roman letter. Thus, the circumstance that these shepherds are always under fever, shows a radical difference between malarial and non-malarial fever. Typhoid fever, belonging to the latter group, occurs only once in a lifetime. After going through it, the inured human system may be exposed to its

causes without effect. The infectable protoplasm has either been fermented out of the system, or, in some inscrutable way, altered, so as to be no longer susceptible of the action of the infective agent.

Here, then, is a marked specific difference in the two forms of fever—the malarial and the acute specific fever—the former connected with the soil, the latter only with the living body. Between them there is no intermediate shade, except in some eclectic pathologist's fervid imagination. In nature there is no known transition from the one fever to the other. The malarial fever is quite unlike the typhoid fever abounding in the Roman capital. The citizen who there may have passed through typhoid fever, can live on through life exempt from further risk; but, if he venture to sleep on the Campagna, he will quickly be attacked with malarial fever, and continue to have recurring fits of it as long as he there resides. To both fevers, therefore, man alone is prone, while cattle are liable to neither; and each fever requires a totally different method of prevention.

The low moral degradation entailed on the Campagna shepherds referred to in the letter, cannot bring their humanity any nearer to the brute in pathological affinity. The presence in the body of the one, and absence from the system of the other, of particular infectable elements, discoverable by no chemistry, or microscopic vision, or physiology, forbid the possibility.

By inherent peculiarities of physical structure or vital endowment, differentiating man etiologically from all other animals, there is formed a gulf dividing the human from the brute organism that no bridging evolution theory has hitherto been able to span; while it is, to say the least of it, not a little remarkable that the highest problem in human nature should be most nearly approached, not through the pathways of ontology, or physiology, but simply through the meanest, vilest, and most humiliating morbid condition.

By his fevers man is an epitome of all animal life, and something more besides—a more than morbid paragon.

But all these considerations have only a

conjectural bearing on the question under review.

All that the requirements of public sanitation demand is prompt decision on the disputed question of contagion. According as opinion may be valid or invalid, so will public health escape or suffer. None doubt that specific contagion is the only mode of propagating allied fevers among cattle, wherein are no discoverable bacteria; and better reasons than any hitherto assigned ought to be given for hesitating over their prototype.

But while theoretically wrong, the reviewer does not appear to have practically tried the preventive power of alum, either in exantheme or enantheme acute specific fever; his objection, being neither scientific nor empirical, can only be an idol; nor is he ingenuous enough to acknowledge that nowhere else can he find reasoning on details like that offered in the work, for using alum to destroy recent typhoid virus.[1] If he can, the sanitary world

[1] In an experiment lately made on the disinfection of enteric excreta, Dr. John Dougall, of Glasgow, tried the effect of permanganate of potash, in the form of Condy's

will be the greatest honour for information of which he appears the custodian.

It therefore behoves on the opponent to say—

'It would be out of place for us to consider the opinions 'expressed in the work as the subject of treatment.'

The question of treatment, medicinal and preventive, is only touched as far as it helps to ameliorate the mystery of etiology, and gives an increase to principle of detail. It is that it enters into the very heart and gist of the discussion. To tends to prove human a germicide and the fever a mere germinal. For if a living animal poison is killed within the body. This theory of the *modus operandi* of

fever, and hence that before the enemies would destroy the organic ... *in animal*, not *agenesis* matter in the system first and some passed in a week by the patient. Two hundred and eight ounces of Condy's fluid were required to oxidise the urine, and eight hundred and forty ounce ounces to oxidise and dissolve the weeks faeces, making in all about 5½ worth. A ordinary process to disinfect the discharges from a single patient for one week. A letter often could have been produced on the organic ... animal matter is less than five shillings worth of alum.—See *Permanganate Fluid*, Soc. Glasgow, 1874.

quinia is made clear in a sentence. The distinctions betwixt the physiological property of quinia to lower temperature, and the therapeutic power of the drug to cure disease, are matters of quinia dosage that no author should overlook or critic confound.

The treatment of typhoid fever by small doses of quinine, given at brief intervals, is advised, because quinine is known to lessen the number of white blood globules, by which morbid poison is now supposed to travel; and, also, because the alkaloid is, at the same time, broken up in the system, to serve as fitting nutriment to the fever famishing body. How quinine fulfils the latter purpose cannot well be understood; but that it does so can hardly be longer doubted. At least such would now appear to be believed, from a brief article in *The Lancet* of 7th September last, evidently prepared about the time the book was written, and offering the same explanation of the operation of quinine as is therein given. Referring to "*The Assimilation of Quinine*," the English periodical states that the amount of quinine destroyed in the body has been studied

by M. Personne, who concludes that "about "one-half of that taken in is destroyed;" that is to say, one-half the quinine is assimilated, and its elements appropriated by the system.

Again, the credulity of unbelief displayed by the reviewer appears in rejecting evidence of fomites-conveyed contagion to the Malmsbury family, for he thinks it

> "By no means carries conviction, as it is much easier to "believe that the disease was acquired in Melbourne, or "some other place *in transitu*, than that the infection had "been conveyed by fomites in the clothes of the son, who "had the disease months before in America."

But there is direct testimony proving the arrival of the family at their journey's end in perfect health, and their so remaining until *after* the soiled garments were washed; and, further, that the girl who handled the infected apparel became first victim. She alone of all the family was exposed to their contained contagium. From her others sickened. Had fever been caught *in transitu*, all equally exposed in travelling would have fallen ill together. That view the sequence excludes. Handling, in Victoria, linen soiled by typhoid dejecta in

America, and an outbreak of fever, may have been only a coincidence, and reasoning on it be of the *post hoc ergo propter hoc* order. If, in lighting her fire for the boiler, the laundry girl had caused a bush fire, would the sophist suggest coincident spontaneous ignition from sun rays condensed through the burning lens of a broken glass bottle? The reasoning would be no more specious. None deny a power in fomites to light a fire in the blood as readily as dried chips take a spark from tinder. Here were fomites and ready blood at Malmsbury brought together. No other assumption is needed. Contagion in typhoid fever not being a favourite doctrine in America, it is little probable that particular care would be taken to disinfect the patient's clothing. And all the reviewer's other objections to the instances of carried contagion could be equally refuted.

Of cases given in proof of carried contagion, it is observed that "In our opinion there is "something defective in the evidence." What the defect may be is not stated. But when it is added that "most of the cases were given "in the report and in the previous edition," a

material fact is kept back. Many of the cases were left defective through reticence of people refusing information. Those deficiencies have been by perseverance at length made good. Missing links are now authenticated by names before, in deference to their owners' wishes, withheld, a courtesy causing the invidious to hint there were no names to adduce. Isolated cases, or rather, cases *apparently* isolated, have been connected with one another, making a perfect chain of evidence, continuous and complete. This, the very end and purpose, the *raison d'être*, of the new edition, is explained in the preface to the *Report*, but not in the review. Moreover, in one particular new case, the fact of carried contagion was independently investigated and verified by Mr. Le Capelain, who was not informed to what end he made inquiry, and was incapable of being influenced even if he had been told. That case was originally published subsequently to the former edition *in the reviewer's own journal*, and under circumstances that loudly called in candour for that journal's very special recognition.

Then, in referring to the alleged peculiarity

of air-borne typhoid contagion, common in this country, the reviewer asserts that

> "Mr. Thomson does not attempt to supply proof, and "we are the less willing to accept his mere word, in the "face of the large body of unimpeachable evidence, that "typhoid fever is often caused by exhalations from cess- "pools or drains, or by the use of water or milk contami- "nated with sewage matter."

The "attempt" *is* made to show this to have been the mode of conveying contagion at Elsternwick and Kew. Whether the "attempt" will be successful or not in carrying conviction to the sceptical mind, may depend as much upon willingness as upon ability to weigh evidence. But it cannot truly be said that the author has not made the attempt. "The "attempt, if not the deed, confounds" the critic. In disputing that effort to explain what has not otherwise been cleared up, can or will the doubter now adduce authentic evidence of a solitary instance, found since the date of the cited text-book, by Ballard or Buchanan in England, or Russell in Scotland, where typhoid fever was propagated "by the use of water or "milk contaminated with sewage matter"

minus specific virus derived from a fever case? that is to say, without direct contagion. If he cannot, or will not, his remark stands not unimpeachable as a trebly false suggestion: unfair to the author, ungenerous towards many who ably conducted convincing inquiries in England and Scotland, and, infinitely worst of all, unfaithful to the subject.

In the *Practitioner* of September last Mr. J. Netten Radcliffe, in an address delivered to medical students at the Royal Naval College, gives a summary of cases, referred to in the *Report*, where typhoid fever had occurred from breathing sewer air, or drinking milk, or water defiled by no ordinary filth, but with specific poison cast off from inflamed bowel glands of persons in typhoid fever.

That paper may therefore be consulted to enable the journalist to meet the proffered challenge, fortify an untenable position as far as it can be logically held, or candidly concede what all will doubtless now feel must eventually be abandoned.

Perhaps the most instructive narrative, and bitter satire on modern civilisation, is where

the tale relates how young children in refined London nurseries were killed by being fed on "nursery milk" fetched from the favourite "one cow" at a country dairy where a dairyman lay in typhoid fever, and morbid poison shed from his diseased bowels mingled with the well water used to "*clean*" the milk pails. The cows were healthy and the pasture good, no diluting water reduced the strength of the milk for the lactometer, and yet that milk was made impure in a way past finding out by test tube or microscope, for the poisonous impurity could only be determined by a *pathogenic* or fever-producing property. The long used drain water never did appreciable harm until specific typhoid fever morbid poison was added; and then it did deadly work fifty miles away. The fever was not *contagious* to persons in direct *contact* with the sick man; but it was as contagious as small-pox to unsuspecting children living in a far distant nursery.

If experimental philosophers had tried the poisonous effect of those morbid discharges, not on pigs, as proposed by our local *savans*,

but on human beings with deliberate intent to excite typhoid fever, the outcome of their tentative efforts could not have yielded more exact results.

Nor is it likely that more definite proof of the contagious nature of typhoid fever ever can be obtained by observations in isolated households in the Australian bush. That cherished notion turns out, after all, illusory. Dense populations afford better facilities for tracing the course of typhoid fever virus than thinly peopled places. All that has hitherto been wanting was the clue. As long as the wild cry about sewer air was kept up, the din deafened all hearing of reason. But now that the uproar and clamour have suddenly ceased, the mental ear opens to quieter sense, and may guide to more deliberate action.

With a consensus of opinion now prevailing on the contagiousness of typhoid fever, from Murchison, the highest medical authority, to Tyndall, chief scientist, it would be nothing less than suicidal to delay prevention. The public have no longer to wait for orders from official authority. The second inquiry set afoot

to find out what the first was alleged to have failed to discover, has, like its predecessor, found plenty of proof positive of contagion, with some of the usual inferential negative appearances of pythogenesis. That *Report* was remitted to its author for emendation, and has been by him amended; but it has not yet been made public. The Central Board of Health would fain put off awhile until somebody could find proof of the origin of typhoid fever independently of any prior infecting case; but that forlorn hope is now finally abandoned in despair, and all thought of further prosecuting the inquiry into the etiology of typhoid fever, from their point of view, relinquished. The Secretary is to furnish no further report on typhoid fever. The tempting intimation given to members of the medical profession, that "The Government of Victoria were willing to "pay liberally for more information," not on etiology, but on "experiments conducted in "this colony to prove the possibility of the "origin of typhoid fever without a prior case," has proved of no avail, and the *liberal* Government of Victoria will not now be called

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The original inquiry having been thus on every hand corroborated the matter may now be considered theoretically settled, and the time for practical activity come. Indeed the Officer of Health for Melbourne, acting under the auspices of the Local Board of Health, issues a bulletin advising the inhabitants of the City, and through them every one in the colony, that "*The poison of Typhoid "Fever lives in excrement, and in other discharges "from the patients*" suffering from that disease.

"*so far as experiments went in this colony, to show that "people took typhoid apart from any possible infection.*"

An engraved circular, sent from the Central Board of Health to every medical man in the colony, on 17th October, 1877, intimated that a fit gentleman had "been specially deputed to make the proposed investigation;" and the *Progress Report* which the Board have had in their hands, and under cogitation for many months, afraid, ashamed, reluctant, or in no hurry to publish, or about the merits of which they are unable to make up their minds, is the result. If this report had made out a case in favour of pythogenesis and the Board's proposition, there would not have been the same detention.

Such is the dilatory fashion of conserving public health in a young country where typhoid fever prevails more in proportion to population than in the oldest and most fever-stricken haunts.

After that candid admission of the now fully demonstrated true etiology of typhoid fever, the grand principle contended for in the *Report* has been conceded, and all agreeing and working well in harmony, the people of Melbourne need no longer suffer from typhoid fever at a rate three or four-fold that of communities governed by boards of health, who strive to control contagion.

Deprecating technicality, and extolling free paraphrase as more appropriate for exposition, it is, in the review, objected

"That the most technical expressions are always selected and that in a book ostensibly intended for general perusal."

The *Report* was adapted and addressed to the peculiar wants of the medical members of the Central Board of Health, who required to be informed on a scientific method of preventing typhoid fever, which had to be discriminated. In deference to the representative status of the instructed, the lesson was couched in scientific terminology such as educated medical men read easily without aid of glossary. The necessity for technical terms to make discussion complete, is shown by the reviewer's

own use of the word "pythogenic," where the homelier equivalent "mud-bred" would have equally suited the context.[1] The book was not "ostensibly intended for general perusal." A society of laymen, aided by the author, long fought against and repelled an invasion of deadly rinderpest, securing their own fortunes with the country's welfare. To them is the book dedicated, hopefully to incite medical societies to emulate their example.

If the reviewer be unaware of the connexion,

[1] "That knowledge which is new and foreign from "opinions received, is to be delivered in another form than "that that is agreeable and familiar. For those whose "conceits are seated in popular opinions, need only but to "prove or dispute; but those whose conceits are beyond "popular opinions, have a double labour: the one to make "themselves conceived, and the other to prove and "demonstrate. So that it is of necessity with them to "have recourse to similitudes and translations to express "themselves. And, therefore, in the infancy of learning, "and in rude times, when those conceits which are now "trivial were then new, the world was full of parables "and similitudes; for else would men either have passed "over without mark, or else rejected for paradoxes that "which was offered, before they had understood or judged." *Advancement of Learning.*

he may learn from the journals how "to "Mr. Thomson, who from the first advocated "total prohibition, and warned the advocates "of quarantine and disinfectants of the futility "of those precautions, is chiefly due the "passing of the Act which has so admirably "answered the purpose; to his powerful "advocacy, also, the Colony partly owes the "extension of the operation of the Act from "time to time, notably in 1874, when "Mr. Alexander Bruce, the chief inspector "of stock in New South Wales, recommended "that the ports should be opened."

Opposite counsels have, unfortunately, now prevailed; but the National Agricultural Society adheres to its wiser policy. An oblation is made to propitiate political gods. In admitting the sacrificial animal, every one prays it may not conceal the dread enemy. The end of the first lustrum leaves the Colony clean of cattle fever. The same period finds it full of typhoid. If the close of the second become less prosperous, the grateful National Agricultural Society may earn another dedication. So will the Central Board of Health if they

yield to act upon now matured conviction, and award the honour of their conversion to whom that honour is due. Acute specific fevers in cattle are never allied to dirt, but are always attributed alone to direct contagion; and no more should contagious fevers in human beings be otherwise associated.

The London *Veterinary Journal* thinks the sea voyage long enough to ensure safe transit of stud stock free from risk of conveying contagion, forgetful that a stud cow brought hither pleuro-lung fever, and a stud bull epizoötic aphtha. To Australian stockmasters the *Veterinary Journal* will therefore be an unwary guide. The calamity endured from imported rabbit infection is a bagatelle compared with the effect of imported cattle plague, to be shortly brought hither, like another acclimatisation foible, under the unguarded energy of a too utilitarian enterprise.

In sympathy not unnatural for a compiler, the reviewer remarks that

"The tables, which have been compiled by Mr. Moerlin "from the records of the Melbourne Observatory, will, "however, repay careful study, and may yet supply

"something more exact than the deduction that hot and "dry seasons are associated with much typhoid fever."

The valued tables were skilfully compiled for their inventor by the custodian of the data. One was architect, the other builder. To whatever other deduction their inductive facts may lead, the reviewer may have no notion; but he surely would never unkindly begrudge a thought original for their immediate application; neither can it be said that with regard to the urgency of the case, that application would be premature.

Similar was the fate of the author's *Phthisis Tables*, now adopted by the Government Statist. Their force and fidelity "produce conviction and command respect," even in those who at first contemned them and reviled their inventor as a "slanderer of the colony;" while they enable the Government Statist, in last *Year Book*, to set right the errors of the Committee appointed by the Medical Society of Victoria, to inquire into the realities shown by the data contained in the startling tables. When the equally genuine *Typhoid Tables* are embodied in next

Year Book, they too, may be admitted to confer another boon on the community.[1]

[1] Referring to these *Phthisis Tables*, *The Argus* of 6th September, 1877, says that at a meeting of the Victorian Medical Society, attention was called to "certain observations in a recent *Lancet*, in which some "important misstatements in Mr. W. Thomson's last "book on *Phthisis in Victoria* were given currency to." It was also declared to be "the duty of the Society "to contest the disparaging slanders of the colony, for "which there was no sort of justification; and further "suggested that the Society should take up the matter, "and report officially upon it." Accordingly, it was again announced in *The Argus*, on the 4th of October following, that the Society had determined to submit the investigation of the subject to a special committee, who were desired to bring up a report. The result of the inquiry was next given in *The Argus* of 6th December of the same year, to the effect that at a meeting of the Society, with an "attendance unusually large," the report of the committee appointed to investigate the subject was brought up, and found "most complete and comprehensive." Among the conclusions come to were:—

1st. "The rate of mortality from phthisis in Victoria has been perceptibly less of late years."

2nd. "The apparent increase of mortality among young "adults is due to the influx of phthisical persons from "abroad."

3rd. "The reduction of the mortality among young "persons is to be explained by a comparative immunity "among those born in the colony."

To refute the 1st conclusion it will be enough to note

On the ambiguity in fever nomenclature prevailing prior to the inquiry, it is apologetically observed in the review that

"Now, there is not the same 'chaotic condition;' "whether the improvement has been brought about by "the report or by the use of the new editions of well-"known manuals, it is needless for us to say."

It is equally superfluous to add more than the following words from the last issued *Year Book* of the Government Statist, 1878, where, at page 115, it is stated that "Deaths from phthisis in 1877 numbered "1,088, which is a larger number than was ever pre-"viously recorded in any year." Phthisis was at the head of the list of causes of death, "the most destructive of all complaints."

These remarks refer to the whole colony. But "the "death-rate from phthisis in Melbourne and Suburbs is "much higher than that of the whole colony, *and* "*increases from year to year.*" "*It approximates closely* "*to that of England and Wales.*"

The increase in deaths from phthisis per 10,000 persons living in Melbourne and Suburbs, was from 20·51 in 1873, to 22·74 in 1877.

The words "*and increases from year to year,*" are not in italics in the *Year Book*, but they are italicised here because they bear out the statement in the tables that were, by the Medical Society of Victoria, accused of "slandering the colony." The Medical Society do not revile the Government Statist for "slandering the colony." "*Non res, sed displicet author.*"

The 2nd conclusion, "that the apparent increase of

that the inquiry found chaos, and that out of chaos came order, immediately after the publication of the *Report*. The reasoning may be another instance of *post hoc, ergo propter hoc;* but the fact of sequence remains indisputable.

"mortality among young adults is due to the influx of "phthisical persons from abroad," is at once confuted by the table in the *Year Book* showing that "in the great "majority of cases the period of residence in the colonies "was of much longer standing than the complaint, "thereby proving that the latter had been contracted in "Australasia. Thus, of the 593 deaths of persons born "outside the Australasian Colonies, respecting which full "particulars are specified, as many as 569, or 96 per "cent., contracted the disease in Australasia, and no "more than 24, or 4 per cent., contracted it elsewhere."

"The duration of illness in connection with the period "of residence in the colonies," as shown in the table, "is now," says the Statist, "given for the first time." But the Statist omits to add that the same table, proving the same truth, was given for the first time in my "*Digest*," &c., published in 1870. Had that table been earlier adopted by the Government Statist, it would have spared the Medical Society of Victoria and *The Argus* from falling into error.

The 3rd and most important of the three assertions, viz., that "The reduction of the mortality among young "persons is to be explained by a comparative immunity "among those born in the colony," can be equally easily rebutted. This can readily be done by the following

In painfully didactic tone the reviewer calls

"Attention to the wise words of Dr. Stokes, which "might be substituted with advantage for one of the "many mottoes which the book contains:—'I would "advise you, as a general rule, not to range yourselves "among the advocates of any exclusive doctrine.'"

He is evidently unaware that, for writing the

figures, obtained from the Registrar-General, Mr. Gibbs, who is the custodian of the data, and under whose verifying supervision they were extracted from the general returns by Mr. Fenton, the most experienced clerk in the vital statistics department. The table would probably be found another useful addition to the next *Year Book*, for which purpose the compiler of that work is to it again made heartily welcome.

RETURN *showing the Estimated Mean Population and the number of Deaths from Phthisis in Victoria during the Years 1871 to 1877 (both inclusive), distinguishing those of Persons born in Victoria, Tasmania, the other Australasian Colonies, and Other Countries.*

Year.	Estimated Mean Population of Victoria.	Total deaths from Phthisis.	Deaths from Phthisis of persons born in				
			Victoria.	Tasmania.	Other Australian Colonies.	Total Australia.	Other Countries.
1871	738,725	841	81	7	13	101	740
1872	760,991	876	97	19	10	126	750
1873	780,362	945	129	19	16	164	781
1874	798,688	1,011	137	21	21	179	832
1875	815,034	1,027	209	15	25	249	778
1876	830,679	1,010	150	16	27	193	817
1877	849,873	1,081	205	30	27	252	836

unlucky chapter, the late Dr. Stokes lost caste as a pathologist. Never was Nemesis more prompt. The great "advocate of an exclusive doctrine" of typhoid fever pathology would "lose caste in the ranks of science." In Lecture IV., on "*Continued Fever*," typhoid

The above table includes only deaths registered as from "Consumption," "Phthisis," and "Hæmoptysis," but none from "Tuberculosis," the number of which on the return amounted to 77 of the native-born; and most medical readers will be inclined to add them to the list of deaths from phthisis.

This table therefore plainly enough displays how the ratio of phthisis among the native-born white population rose gradually from 12 per cent. of the total deaths from phthisis in 1871, to 23 per cent. in 1877. Whether this increased ratio corresponded to the increase of native-born persons in the mean population, or not, cannot be known before the next general census of 1881; but meanwhile the steady increase gives no warrant whatever to affirm that the young native-born white adult enjoys in this colony a remarkable immunity from phthisis, as is asserted by the Medical Society.

If, therefore, between the two journals, *The Lancet* and *The Argus*, there has been any misleading, it has not been *The Lancet* but *The Argus* that has been led astray, and induced to "give currency to disparaging slanders for which there was no justification."

It may appear foreign to the subject of typhoid fever to refer at length to phthisis; but as the accuracy of data given for each disease was impugned by the same

fever is denied to have any special anatomical lesion. Therein Stokes opposed the teaching of Jenner, who had a few years before brought order out of conflict, discriminated *typhoid* from *typhus*, and made possible a scientific prevention. From the precision of Jenner, Stokes would have had the profession relapse into the empirical, to be followed by our reviewer back into the dark abyss of pathological ignorance.

The rather cynical allusion to "typhoid, or the so-called pythogenic or enteric fever," of Jenner and Murchison, may have been only in the peculiar vein of Stokes, giving an

critic, it is convenient to defend both at once, and save trouble and a separate publication.

Responding to an admonitory tone in the review, it may be added in words used by a contemporary author, whom *The Argus* will allow to be a keen and fair debater, that:—

"All defence is reassertion, which properly requires "a deliberate reconsideration; and no man who thus "reconsiders should scruple, if he find so much as a "word that may convey a false impression, to amend it. "Exactness in stating truth according to the measure "of our intelligence, is an indispensable condition of "justice, and of a title to be heard."—GLADSTONE, *The Vatican Decrees.*

"example of the modesty of science and the "practice of a reverential spirit towards "those whose works prove that they were "great and truthful men." But there can be no mistake or qualifying phrase about the hearty contempt that made Jenner retort and tell the Clinical Society "there are some "questions that admit of settlement, and that "they are not settled is discreditable to us "as a profession." The exact etiology of typhoid fever was primary in the number. For pleading for rigid theory in etiology, Jenner was ranked, in the estimation of Stokes, as one of those who would "lose caste as a pathologist." Yet there are probably not in the medical world beyond Melbourne three medical men who have not long ago rejected the multiple causes and respectable conservatism of Stokes, and accepted the rigid unity of Jenner, the thorough pathological reformer.

Another writer, whom the reviewer wrongly cites as a non-contagionist, has just given the means of rectifying that mistake. In the last issue of the *Proceedings of the Philosophical*

Society of Glasgow, 1878, a paper by Dr. J. B. Russell, Medical Officer of Health, Glasgow, on *Filth Diseases in Town and Country*, contains these words:—

"I can say of my own knowledge, from "investigations into the enteric fever of the "last four years in Glasgow, that when it has "become locally epidemic in certain parts of "the town, it has been derived from excre- "mental pollution miles off, either demonstrably "or with strong probability. So it has been "in Greenock, in Edinburgh, in Leeds, Lon- "don, and many other towns. . . The coun- "try contributes such diseases to the town. . .

"The town and its arrangements are credited "or discredited with much enteric fever, at "any rate, with the genesis of which they "have nothing more to do than with the "thistle-down which is borne into the streets "from the country meadows," causing what Dr. Russell aptly calls "thistle infection."

In a case investigated by Dr. Russell, and related at page 264 of the *Report*, it was perfectly evident that the excremental matters polluting milk supplied to Glasgow consumers

came from a person lying ill of typhoid fever at the supplying dairy-farm away in the country.[1]

[1] Of the propagation of typhoid fever by infected milk, Mr. Kingzett says: "Taking, therefore, a general "survey, it cannot be fairly said that the parasitic "theory, as ordinarily explained, is explained correctly. "The grounds upon which this theory is based are "indisputable, but there is an almost universal tendency "exhibited to exceed the legitimate inferences to be "made from them. This fascination has led many into "error, and in it is to be found the origin of what may "be called public 'scares.' Thus, of late years, an "attempt has been made to connect the causes of "typhoid fever with impure milk, containing, as it is "supposed, virus matter derived from sewage or other "effete substances."—*Animal Chemistry*, 1878, p. 439.

Here Mr. Kingzett's logic is entirely at fault in mixing up two distinct problems, viz., whether typhoid virus consists of germs in the form of animal or vegetable cells, or of albuminous matter derived from liquid evacuations of the fevered; *and*, whether such virus can be derived from ordinary sewage or effete matter, or alone when these are themselves contaminated with specific typhoid virus from a typhoid fever case. Now, whether that modification of Liebig's theory, which has been lately mooted by Mr. W. Stewart in the *British Medical Journal* of March 10, 1877, as cited by Mr. Kingzett, and which the writer is inclined to adopt as the most agreeable to fact, be found true or not, certain it is that in all practical inquiries referred to, the leading point kept in view was not as to the nature or form of the virus, but only whether it came out ordinary sewage, or direct from a fever case; the l

It is further shown by Dr. Russell, that, "as it were under a penalty of death, the "towns have by sanitary works, *and the pre-* "*vention of infectious diseases, so controlled the* "*channels of conveyance*" of contagion, that, in regard to enteric fever, a citizen of Glasgow runs less risk of dying of that disease than an inhabitant of any rural district. And he also adds—"unless when an epidemic in the "country discloses itself by sending sparks "into the midst of a large town, or when a "local correspondent is sufficiently independent "of local influences to let the truth out in a "city newspaper, the mass of the public have "no opportunity of ever knowing what is "going on."

Therefore is Dr. Russell not only no *pythogenist*, as is stated in the review, but rather a strict contagionist, as the *Report* represents;

fact being made out, clearly established the theory of transmitted contagion, and thereby completed the work of the practical sanitarian, remitting the ultimate problem of the form of the virus for settlement by the pathological chemist, to which end some additional remarks on the chemistry of typhoid fæces to those given in Mr. Kingzett's book would have been acceptable.

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but he is also a most advocate of full publicity, deprecating everything like burking or disingenuous suppression.

In Dr. Russell's paper, much other information is given, valuable for comparison with Victorian records. Thus, in a tabular statement of the death-rates in eight large towns in Scotland, during an average of eleven years, it is shown that in not one of them is typhoid fever so fatal as it is in Melbourne, being, at per 10,000 persons living, in the following order of severity:—

DEATH-RATES PER 10,000 IN EIGHT LARGE TOWNS IN SCOTLAND, ON AN AVERAGE OF ELEVEN YEARS, AND IN MELBOURNE FOR A SIMILAR PERIOD.

	Typhoid Fever.
Perth	2·30
Leith	3·50
Dundee	3·80
Edinburgh	3·80
Aberdeen	4·50
Greenock	4·70
Glasgow	4·90
Paisley	5·30
Melbourne	7·26

The highest recorded rate in Glasgow up to last year, was that shown in 1868—viz., 6·10; while the highest recorded rate in Melbourne

was, as already stated, that shown in 1877—viz., 10·00 deaths in 10,000 persons living.

The town of Paisley shows the largest average in Scotland; but even it is far below the Melbourne rate, or as 5·30 are to 7·26.

Of the cause, Dr. Russell says: "Paisley is essentially a midden town;" just as Melbourne is essentially a miniature midden city. Neither has a water carriage system; each has an abundant and good water supply; and both are without underground drainage. To bad or infected water, therefore, cannot be ascribed the excess in both places of typhoid fever; nor to sewer air entering houses. But neither can it be in Paisley attributable to want of drainage or wet subsoil, because the mainland rural ratio was higher than the highest town rate. In Melbourne, on the contrary, the rate is higher than for the rest of Victoria. Nor can it be said that a higher average temperature in Victoria may account for the higher rate; because Scotland is worse than England, although there the temperature is generally cooler.

The tenor of Dr. Russell's paper is to explain

those differences, through negligence respecting the contagiousness of typhoid fever; and he shows, as is also shown in the *Report*, how greater density of population, other things being equal, only facilitates the propagation of the specific poison. Typhoid fever is more a rural than a town disease in Scotland; but it is more town than rural, in Victoria. If the latter were equally populated to area with Scotland, and the same theoretical and practical indifference about the contagiousness of the fever continued to prevail, then would the Victorian rural exceed the Melbourne ratio.

Typhoid fever is no doubt a filth-fever, and not an undrained subsoil fever, alike in Scotland, and Victoria; but in both countries it is evident that to propagate that fever, with the specific germ the filth must be itself infected. In the bad practice of a false theory, it is the custom to cast the excreta of the sick undisinfected into the midden, in Paisley, and into the middenette in Melbourne, and hence the all-sufficing reason for a prevalent excess in both towns of an easily controlled fever.

In the former edition of this *Report*, reference occurs, at page 14, to "points "of diagnosis raised by Dr. Perry and "Dr. Stewart, long ere they were touched "by Sir William Jenner." As this statement was not accepted by critics on the author's "mere word against unimpeachable testimony" for Jenner's priority; and as the author had not at hand data to prove his assertion, it not repeated in the third edition. But it may now be re-asserted, since Dr. Russell says that "In 1836, Drs. Perry and Stewart "demonstrated the points of difference (between "typhus and typhoid) in the wards of the "Royal Infirmary;" or many years before the date of Jenner's classic paper.

Another reviewer of a somewhat more generous turn, one who keenly appreciates the difficulties of this intricate subject, admits that "owing to its vast importance it may well "bear the strain of a good deal of dis-"cussion." Whether that remark was intended to imply that its discussion here has been over-strained or not, it is not for the author to infer. Yet, since the obser-

vation has been made, the reviewer's attention might now be called to an article in the *Nineteenth Century*, for November, 1878, by Professor Tyndall, replying to the famous address by Professor Virchow, on *Evolution*, in which the bearing of the doctrine of a *contagium animatum* towards philosophy and science generally, is eloquently set forth. "I write," says Professor Tyndall, "thus with "the theory of *contagium vivum* more especially "in my mind, and must regret the attitude of "denial assumed by Professor Virchow towards "that theory." . . . "By his unreserved "denial," adds Professor Tyndall, "he," Professor Virchow, "quenches the light of proba- "bility which ought to guide the medical man." This guidance cannot be affected by any discovery tending to show that not *fungi*, but only some form of specially poisoned protoplasm is the particular form of *contagium vivum* operating in some at least of the contagious fevers, such as typhoid fever, as affirmed in the *Report*. Until *fungi* be found Virchow refuses to admit their presence. Herein at least an effort has been made to show that

fever phenomena can be explained without assuming their existence. At any rate we cannot here be said to strain a problem which the highest intellects grapple.

The following extract from the *British Medical Journal*, 21st September, 1878, will fittingly conclude the foregoing inquiry by a wise and practical suggestion well worthy of adoption :—

"THE SANITARY INSPECTION OF DAIRY FARMS."

"Even the few remaining sceptics as to the desirability "of the farms from which our daily supplies of milk are "drawn being under some sort of external supervision, "must have been convinced of the error of their ways by "the recent outbreak of typhoid fever at Bristol. Not "fewer than nine deaths, and considerably more than a "hundred cases, of this disease have been traced by the "medical officer of the city, with unerring certainty, to "the use of milk from one particular source, which had "become, in some way or other, poisoned by the germs "of typhoid fever contained in the evacuations of a young "lady suffering from that disease at the farm from which "all the mischief sprang. The investigations which have "recently been made by the officials of the Local Govern-"ment Board themselves must almost have had the effect "of rousing that torpid body into some action with regard "to this matter; and we were prepared, therefore, to wait "until the next Parliamentary session, in the hope that, "meanwhile, some well-considered Bill, dealing with the

"question, would be matured by the authorities who are "supposed to have charge of the public health of the "country."

Than these remarks, nothing could be more opportune for our own wants in this colony.

Particular care has been bestowed on this review, because, revealing very defective information on the recent growth of typhoid fever etiology and pathology, it affords a favourable opportunity of materially enlarging the primary discussion.

FINIS.

I did rather yieldingly accept than forwardly embrace this labour imposed upon me.

BACON

If you think much of my plainness, take heed how you give me authority another time; for I shall do the like.

ALLEN

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